F 21

Alcohol, Drug Abuse, and Mental Health Administration

Perspectives on Family Research

Report of a Workshop

NEWARK PUBLIC LIBRARY

NOV 3 1962

U. S. GOVT. PUBS.
DEPOSITORY 376

U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Public Health Service

HE 20.8002:

Perspectives on Family Research

Report of a Workshop

Edited by
Barbara Gray Ellis, M.S.S., M.P.H.
Division of Prevention and Treatment Development
National Institute on Drug Abuse

U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Public Health Service
Alcohol, Drug Abuse, and Mental Health Administration

The opinions expressed in the proceedings are the views of the workshop participants and do not necessarily reflect the official position of the agency or any other part of the U.S. Department of Health and Human Services.

All material appearing in this publication is in the public domain and may be reproduced without permission from the agency or the authors. Citation of the source is appreciated.

DHHS Publication No. (ADM) 82-1213
Printed 1982

Contents

	Page
Participants in ADAMHA Family Research Meeting	iv
Members of ADAMHA Family Research Work Committee	v
Introduction	1
Part I: A Sampling of Family Research Issues	4
Etiology	4
Family Systems and Family Life Cycle	4
Family Influences	6
Alternative Family Types	7
Approaches to the Study of Family	8
Outcomes	10
Mechanisms and Methodologies to Facilitate Collaboration	11
Part II: Final Session and Recommendations	15
References	39

READEX FICHE

Participants in ADAMHA Family Research Meeting
January 29 - 30, 1981

Sandra B. Coleman, Ph.D.
Director, Research and Evaluation
ACT (Achievement Through Counseling and Treatment)
Philadelphia, Pennsylvania

Theodore Jacob, Ph.D.
Associate Professor of Psychology and Psychiatry
University of Pittsburgh
Pittsburgh, Pennsylvania

Sheppard Kellam, M.D.
Professor of Psychiatry and Director
Social Psychiatry Study Center
University of Chicago
Chicago, Illinois

Thomas S. Langner, Ph.D.
Professor of Clinical Psychiatric Epidemiology
Family Research Project
Columbia University School of Public Health
New York, New York

Cloé Madanes, Lic.
Co-Director
Family Therapy Institute
Chevy Chase, Maryland

Hamilton I. McCubbin, Ph.D.
Professor and Chair
Family Social Science
University of Minnesota
St. Paul, Minnesota

David Reiss, M.D.
Director, Center for Family Research
Department of Psychiatry and Behavioral Sciences
George Washington University
Washington, D.C.

M. Duncan Stanton, Ph.D.
(Workshop Moderator)
Director, Addicts and Families Program
Philadelphia Child Guidance Clinic
Philadelphia, Pennsylvania

Peter Steinglass, M.D.
Associate Professor of Psychiatry and Behavioral Sciences
George Washington University
Washington, D.C.

Jaime Vazquez, M.D.
Project Director
Family Therapy Project
University of Michigan
Ann Arbor, Michigan

Members of ADAMHA Family Research Work Committee

Rebecca Ashery, D.S.W.
Public Health Advisor
Treatment and Research Assessment Branch
Division of Prevention and Treatment Development
National Institute on Drug Abuse

Salvatore N. Cianci, Ph.D.
Grants Referral and Review Officer
Office of Extramural Programs
Alcohol, Drug Abuse, and Mental Health Administration

Jack Durell, M.D.*
(Advisor to the Committee)
Director
Division of Prevention and Treatment Development
National Institute on Drug Abuse

Barbara G. Ellis, M.S.S., M.P.H.
(Chair)
Special Assistant to the Director
Division of Prevention and Treatment Development
National Institute on Drug Abuse

Thomas Glynn, Ph.D.
Research Psychologist
Psychosocial Branch
Division of Research
National Institute on Drug Abuse

Henrietta B. Hubbard, R.N., M.P.H.
Public Health Advisor
Prevention Services Branch
Division of Prevention
National Institute on Alcohol Abuse and Alcoholism

Vicki Levin, M.S.W.†
Special Assistant to the Administrator for Children and Youth Activities
Office of the Administrator
Alcohol, Drug Abuse, and Mental Health Administration

Barbara Najar, M.P.H.
Public Health Educator
Prevention Policy Branch
Division of Prevention
National Institute on Alcohol Abuse and Alcoholism

Joy Schulterbrandt, M.S.
Chief, Family Mental Health and Policy Research Section
Behavioral and Social Sciences Research Branch
Division of Extramural Research Programs
National Institute of Mental Health

*Now Acting Deputy Director, NIDA.
†Now Special Assistant to the Chief, Mental Health Study Center, NIMH.

Introduction

The workshop* reported in this document is one result of the efforts to foster, where appropriate, research which cuts across the programs of the three Alcohol, Drug Abuse, and Mental Health Administration (ADAMHA) Institutes: the National Institute on Alcohol Abuse and Alcoholism (NIAAA), the National Institute on Drug Abuse (NIDA), and the National Institute of Mental Health (NIMH). Such research could cut across the problem areas with which each of the Institutes is identified as well as across some areas of program responsibility carried by each.

In preliminary discussions about possible projects for joint initiatives, "family" was seen as a common construct germane to all individuals with the problem behaviors of concern to the respective Institutes. It could possibly serve as a conceptual unifier in a search for common antecedents to dysfunctional behavior and thereby guide the more precise design of prevention programs. There was growing realization that family research aimed at only one of the problem behaviors of concern to the Institutes (i.e., alcohol abuse, drug abuse, or mental health) does not alone constitute an adequate approach to understanding family influences on behavior and the implications for prevention. And there was agreement about the potential value of cross-cutting, cross-problem family research endeavors in two broad areas: the study of adaptive and maladaptive patterns of behavior that emerge from the context of the family and critically influence functional or dysfunctional behavior, and studies aimed at increasing our understanding of the common family factors in families where drug and alcohol dependence and mental disorders coexist. As a consequence, a Work Committee (see p. v) was established with membership from the three Institutes and the Office of the Administrator (OA) of ADAMHA, with the charge to explore in some depth the interest of the Institutes in implementing jointly sponsored research studies of family factors associated with dysfunctional behavior.

The term "dysfunctional behavior" refers to and encompasses the behaviors that are the foci of the programs of the three Institutes: alcohol abuse and alcoholism; drug misuse and abuse; mental illness, some psychosomatic disorders, violence and abuse, delinquent behavior such as truancy, running away, etc. Each of the Institutes is deeply committed to and involved in continuing program efforts to enlarge the understanding of the origins of the dysfunctional behavior that is its mandated responsibility in order to plan and implement more productive and cost-effective programs for the prevention of that dysfunctional behavior. For some years within each Institute and the professional community there has been growing interest in the notion of the family as a crucially important interpersonal context in which dysfunctional behavior originates, is main-

*Convened under the auspices of the National Institute on Drug Abuse.

tained, and is effectively treated. This thinking reflects the generic idea that germinal patterns of behavior—adaptive and functional as well as maladaptive and dysfunctional—emerge from and may be modified within the context of the family. Along with these concerns there has been increasing interest in the identification of the commonalities associated with several specific dysfunctional behaviors ("the addictive behaviors" and acting out in adolescence, for example).

Earlier research has tended to explore the relationships between the various kinds of dysfunctional behavior and the individual psychodynamics or characteristics of the identified patient manifesting problematic behavior. More recent investigations, however, have begun to look for understanding of the troubled person in the context of his/her family system, paying attention to factors such as the interpersonal relationships among family members and the function for the family system of specific symptoms.

In reviewing the family-related research work currently underway or previously funded by the Institutes, the Committee determined that the *individual* Institutes had made impressive contributions in advancing knowledge and improving understanding of familial and environmental factors involved in the development and course of *each* of the three problem areas. Most of the studies to date have approached family functioning primarily from a *problem* perspective. What seemed to be underrepresented in the ADAMHA research portfolio was a body of studies that explore patterns of family behavior as they generally relate to adaptive or maladaptive states across the three areas. A possible reason for the lack of this type of study may be the single problem orientation of the individual Institutes. This creates a need for special efforts to coordinate applications having crosscutting themes.

The Committee proposed to stimulate examination of a research perspective that would be different from the trend in prior research, which reflected primary interest in the symptomatic individual, and would focus instead on the family as a complex social system with individual dysfunction as a subsidiary issue. The earlier problem focus would be broadened to include family processes, characteristics, organizational structures, and network patterns. As an essential step in the further development of this enlarged research perspective the Committee proposed to bring about a critical examination by representative family researchers from the professional community of the issues involved in promoting joint initiatives in family research.

Thus, the Committee members invited 10 distinguished family researchers (see p. iv) to join with them for a workshop on January 29 and 30, 1981, to consider (1) the feasibility of joint studies across the three Institutes designed to bring about an increase in understanding of the underlying processes in family function and dysfunction, and their association with individual symptomatology; and (2) the issues affecting investigations of family factors associated with the dysfunctional behaviors that are the foci of the programs of the three Institutes.

As the Committee fine-tuned its planning for the workshop, the focus narrowed to concerns with etiology, to family factors that are antecedents to dysfunctional behavior. Two major but related questions emerged as areas most appropriately examined by representatives of the family research community: What family factors predict family dysfunction and can be seen as precursors of dysfunctional behavior in one or more family members? Can an understanding of generic and specific family factors be turned toward applied research in support of sharply targeted prevention programming?

The Committee set forth a series of subject areas that would serve as guidelines for the deliberation of the workshop participants:

- aspects of family dysfunction associated with and predictive of specific kinds of dysfunctional behavior;
- specific precursor family factors that are associated with and predictive of specific dysfunctional behavior in one or more family members;
- generic family factors that are associated with and predictive of dysfunctional behavior in one or more family members;
- aspects of family functioning that are generally predictive of dysfunctional behaviors;
- common family factors associated with more than one kind, or all kinds, of dysfunctional behavior.

Other subject areas of particular relevance were identified: interpersonal family dynamics; family structure; context (family and environmental); the family developmental cycle; how families process both predictable and unpredictable family events; and family demographic characteristics.

This workshop report is in two sections: Part I is a sampling of research issues identified by participants—as they expressed them—in the earlier sessions of the workshop; Part II contains discussions during the final session and workshop recommendations.

The report represents "straight talk" from family researchers regarding the importance of integrated research approaches and of joint programming. In the months since the workshop, there have been several developments related to these perceived needs. They include:

- Growing appreciation of the value of a broader perspective in the review of grant applications for family research.
- Joint Institute sponsorship of a workshop on strengthening the role of the family physician in the prevention of dysfunctional behavior.
- Progress toward conceptualizing a long-range family research project which would include a series of preliminary research projects.

These activities have helped move the family research field forward and are a step toward achieving a more integrated approach to such research.

PART I:
A SAMPLING OF FAMILY RESEARCH ISSUES

Etiology

DURELL: We are interested in learning more about the etiology of adaptive or maladaptive behavior. We hope to stimulate research that will assist us in understanding these factors so that we can plan more effective preventive interventions.

STANTON: If we perpetuate the kind of thinking and operations that have gone before, then there will be nothing new added by this group because those earlier activities are established. We are looking for something new for ADAMHA and the field in general as culled and synthesized from these discussions.

Is "etiology" a proper term? That word so often implies a linear way of thinking versus recursive or non-linear. I prefer the term "causality," because the term "etiology" is encumbered by some modes of thinking that are constraining.

KELLAM: Etiology doesn't mean one-directional. It means a system of determinants and how they interrelate with each other on some outcomes. We should be looking at causal systems and not doing just descriptive work. Causal models have to begin somehow with description of the family over time, as well as at points in time. Family variations — if you look at the context of a community or society—have to be part of that causal modeling.

Family Systems and Family Life Cycle

STANTON: Perhaps we could briefly discuss various family issues or family factors that are pertinent to the main mission of the Institutes, and begin to think about the kinds of structures in the Institutes that might facilitate examining those issues.

REISS: In a number of respects this meeting represents an important historical juncture. It is a reflection of the awareness of the three Institutes that something new is emerging. It reflects provisional acceptance of what is being loosely called the "systems" viewpoint.

STANTON: Family systems thinking represents a departure from earlier paradigms. Those models produced good studies that are just now beginning to bear some fruit, but they are no longer sufficient for some who want to go beyond a focus on individual pathology. The systems approach and the more traditional approach represent two ways of conceptualizing the origins of behavior. Our tools haven't kept up with the emergence of systems thinking. For example, we can't test over time our observation that a youth's addiction serves the function of holding the parents together. We don't have good curvilinear analyses. In the family field most

of the real breakthroughs—conceptual and otherwise—have come from natural observation, observations made earlier and outcomes assessed later. This is a valuable method and should not be discarded while we are keeping track of what additional leverage this new systems perspective may provide.

STEINGLASS: The issue is more how these two relate, and how and when one flips from one to the other to do the things we ultimately want to be able to do.

REISS: It may be that certain kinds of adolescent problems are less closely linked to family structure, but at the same time there may be depression problems for which there is good evidence in that very same family of close linkage to the family. And if you don't have a family perspective—if you don't use this newer model—you will be missing something of great importance to one of the Institutes, or two, or maybe to all three.

STANTON: A lot of earlier work has been done on the individual life cycle. That is not the charge of this group. We are interested in lifespan developmental aspects of families, in looking at the function of the family with respect to the life cycle and particular outcomes.

KELLAM: Some say the family's function and influence occur early vis-a-vis the child, and that with increasing age the influence of early family experiences diminishes. It may or may not be a useful strategy to influence the family system, by preventive intervention, if its influence diminishes as a function of age. The question of prevention is the question of where along the life cycle you intervene and at what targets. The family can't be presumed to be the prime continuing cause of all subsequent behavior. This is one of the hottest research questions about the family.

STANTON: In studies of heroin addicts, the evidence overwhelmingly indicates ongoing family interaction that becomes stuck at an inappropriate developmental stage. We find life cycle paradigms very useful. It is the family *system* that gets stuck at the "leaving-home" stage. The family system also includes life cycle events that occur in the grandparent generation—such as illness, death, immigration—events and stress signals emanating from the earlier generation. You can't see those precipitating events if you look only at the level of the nuclear family.

Haley made the point 10 years ago that perhaps a more viable approach would be one that depends on developmental stages in the family life cycle, rather than the type-trait kinds of issues we can get into. When you see a cross section of family structures that seem very similar, and some of those families hit snags and some do not, how do you best account for that except timewise or developmentally? You may find the family that hits snags and has problems may be in a different life cycle stage. I've found this way of looking at things to have utility. It opens up a number of areas that haven't been examined, and I think that's where a lot of the variance is in the cross section research.

• • • •

COLEMAN: We need to think in terms of different influential systems. The concept of family as applied to heroin addicts may be a different concept from the concept of family in other kinds of dysfunctional behavior. So if we think of using systems as the basic component of etiology, then that system could vary according to the population. The system can mean school, peer group, intact family, single-parent family, extended family. So we may say that we're looking at this meeting as a way of exploring more in depth the systems that influence dysfunctional behavior.

STEINGLASS: Another way to flip-flop the question is to ask at what point in the family life cycle individuals are less important to the family, or at what point they are more important to the family. That treats the family as the entity under study. And it's difficult for some researchers to conceptualize around the notion of the family as a group or unit. Demographic level studies tend to be based on individual events rather than family events. An area we'd like to know more about is one that deals with transitional events. There is something about development that is very important, and it ties in very closely with the notion of transitional events and the idea of family resiliency or, in biological terms, adaptive capacity for dealing with those events.

Family Influences

REISS: Fundamental basic research not connected with specific pathologic outcomes is absolutely essential. When talking about the rational application of prevention techniques on a family level, there are two levels of basic research that are extremely important: (1) studies of basic mechanisms of the influence of family on personality development, and (2) studies of how various kinds of families respond to outside interventions. There is a budding science of "family environment interaction"—family boundary maintenance—an understanding of how families deal with outside agencies or outside individuals. A thorough understanding of those family maintenance issues would really make any kind of intervention—before and after psychopathology—much more effective because one would be able to anticipate the best kinds of techniques for getting into the systems of different kinds of families. We have good techniques for studying both of those areas.

DURELL: Is the family effect continuous or does it vary over time? That itself becomes an area of investigation for which a number of parameters can be sorted out. When one talks about family influence at a given age, one talks both of the earlier family influence as it affects that age and the contemporary family influence. Presumably, those are relatively independent variables. The *continuing* effect of the family, therefore, is not completely defined by what the effect was at the beginning.

JACOB: Your comments are relevant to the effects of the family of origin on the child as the outcome variable, suggesting that there is another sphere of family influence. When we talk of outcomes in psychopath-

of the real breakthroughs—conceptual and otherwise—have come from natural observation, observations made earlier and outcomes assessed later. This is a valuable method and should not be discarded while we are keeping track of what additional leverage this new systems perspective may provide.

STEINGLASS: The issue is more how these two relate, and how and when one flips from one to the other to do the things we ultimately want to be able to do.

REISS: It may be that certain kinds of adolescent problems are less closely linked to family structure, but at the same time there may be depression problems for which there is good evidence in that very same family of close linkage to the family. And if you don't have a family perspective—if you don't use this newer model—you will be missing something of great importance to one of the Institutes, or two, or maybe to all three.

STANTON: A lot of earlier work has been done on the individual life cycle. That is not the charge of this group. We are interested in lifespan developmental aspects of families, in looking at the function of the family with respect to the life cycle and particular outcomes.

KELLAM: Some say the family's function and influence occur early vis-a-vis the child, and that with increasing age the influence of early family experiences diminishes. It may or may not be a useful strategy to influence the family system, by preventive intervention, if its influence diminishes as a function of age. The question of prevention is the question of where along the life cycle you intervene and at what targets. The family can't be presumed to be the prime continuing cause of all subsequent behavior. This is one of the hottest research questions about the family.

STANTON: In studies of heroin addicts, the evidence overwhelmingly indicates ongoing family interaction that becomes stuck at an inappropriate developmental stage. We find life cycle paradigms very useful. It is the family *system* that gets stuck at the "leaving-home" stage. The family system also includes life cycle events that occur in the grandparent generation—such as illness, death, immigration—events and stress signals emanating from the earlier generation. You can't see those precipitating events if you look only at the level of the nuclear family.

Haley made the point 10 years ago that perhaps a more viable approach would be one that depends on developmental stages in the family life cycle, rather than the type-trait kinds of issues we can get into. When you see a cross section of family structures that seem very similar, and some of those families hit snags and some do not, how do you best account for that except timewise or developmentally? You may find the family that hits snags and has problems may be in a different life cycle stage. I've found this way of looking at things to have utility. It opens up a number of areas that haven't been examined, and I think that's where a lot of the variance is in the cross section research.

• • • •

COLEMAN: We need to think in terms of different influential systems. The concept of family as applied to heroin addicts may be a different concept from the concept of family in other kinds of dysfunctional behavior. So if we think of using systems as the basic component of etiology, then that system could vary according to the population. The system can mean school, peer group, intact family, single-parent family, extended family. So we may say that we're looking at this meeting as a way of exploring more in depth the systems that influence dysfunctional behavior.

STEINGLASS: Another way to flip-flop the question is to ask at what point in the family life cycle individuals are less important to the family, or at what point they are more important to the family. That treats the family as the entity under study. And it's difficult for some researchers to conceptualize around the notion of the family as a group or unit. Demographic level studies tend to be based on individual events rather than family events. An area we'd like to know more about is one that deals with transitional events. There is something about development that is very important, and it ties in very closely with the notion of transitional events and the idea of family resiliency or, in biological terms, adaptive capacity for dealing with those events.

Family Influences

REISS: Fundamental basic research not connected with specific pathologic outcomes is absolutely essential. When talking about the rational application of prevention techniques on a family level, there are two levels of basic research that are extremely important: (1) studies of basic mechanisms of the influence of family on personality development, and (2) studies of how various kinds of families respond to outside interventions. There is a budding science of "family environment interaction"—family boundary maintenance—an understanding of how families deal with outside agencies or outside individuals. A thorough understanding of those family maintenance issues would really make any kind of intervention—before and after psychopathology—much more effective because one would be able to anticipate the best kinds of techniques for getting into the systems of different kinds of families. We have good techniques for studying both of those areas.

DURELL: Is the family effect continuous or does it vary over time? That itself becomes an area of investigation for which a number of parameters can be sorted out. When one talks about family influence at a given age, one talks both of the earlier family influence as it affects that age and the contemporary family influence. Presumably, those are relatively independent variables. The *continuing* effect of the family, therefore, is not completely defined by what the effect was at the beginning.

JACOB: Your comments are relevant to the effects of the family of origin on the child as the outcome variable, suggesting that there is another sphere of family influence. When we talk of outcomes in psychopath-

ology, we think of adult psychopathology outcomes because of the emergence of difficulties in the adult. The relevant circle of influence may be the family of origin, but it also may be the family within which the individual desires to be functional at that point in time. So how do you know which? In studies of another psychopathology—alcoholism—we see onset of that type of maladaptive behavior at a much later point in time for many individuals. So one has to consider family influences of an early nature—family of origin—but one also has to consider contemporary family influence. Another level of important family factors and influence is genetics, family genetic influences, to the extent that there may be some genetic transmission which affects the emergence of certain types of dysfunctional behavior.

Alternative Family Types

KELLAM: Another problem is what the denominator is from which we draw the data? Do we find it at the community level, in the second level clinic population? In the hospitalized or institutionalized population? Epidemiology is immediately called upon to define the denominator. A lot of our past work has been to try to classify variations in family among child-rearing families, such as mother-alone families, mother-grandmother families. Very often we begin with the error of implicitly talking about mother-father families, without ever dealing with the fact that at least half of the families are not so structured. Things may differ depending on who in fact is in the family to begin with. There are so many alternative family types. Mother-grandmother families are different than mother-alone or mother-father families. We need to examine their role differentiation, their impact on child behaviors, and their availability for intervention, if for no other reason than to note high-risk types. So in talking about a family with a young pregnant member, then one with an older pregnant member, we really are taking a developmental cycle approach and differentiating by development stage.

Family membership is constantly changing, evolving. There is an important difference in regard to the mother's life cycle, when she begins her childbearing, whether it is as a teenager, or at 30 years, or older. You ask, for example, how the age of the mother influences her family structure, her age with her first-born, second-born children, and so on.

In regard to childrearing, role differentiation, and process, in what way are mother-grandmother families different from mother-father families and from mother-alone? And how do those differences, taking into account socioeconomic status and demographic variables, relate to child outcome, short-term and long-term? No existing committee* is intrigued by that question. Demographers find it somewhat trivial because it isn't a national sample. The process people don't find this kind of macro—or

*Initial Review Group (IRG).

what appears to be a giant level of measurement—discrete enough. We need some structural changes in these respects, and bringing us together in this meeting is an important beginning.

These are important issues when you consider various ways to approach the study of family. I'm talking about alternatives to mother-father as a basic core definer of family, alternative members who compose the basic unit such as mother-grandmother, mother-aunt, and so on. Then an issue is role differentiation as it differs across those different types; you look at the qualities and quantity of processes that compose the actual interactions. The two traditional ones in that regard are dimensions around affection and rules. There are clearly others. Power in my view is associated with rules. A fourth domain is the influence of socioeconomic and demographic and community variables on this family system, with particular attention to school and the child's evolving course in school. The dependent variable in this set of issues is a profile of child and adolescent outcomes which is both psychological and social adaptational in its content.

Approaches to the Study of Family

REISS: One of the common threads in the papers that we reviewed for this workshop is how to make meaningful distinctions between families qua families and families as groups. I find it useful to think in terms of three different approaches in studying the family: a compositional approach; a process approach; and an approach that emphasizes the search for *underlying*, enduring structure. Each constitutes a conceptual vantage point that a group like this might recommend to be taken seriously by the Institutes because of its potential role in the kinds of outcomes in which they have an interest.

KELLAM: I would agree that one has to approach the family study with all three of those in mind. As a fourth category, I would name the internal social structure of the family in a sociological sense—predictable roles. One can ask how does the mother-grandmother family differentiate rule defining and enforcing in contrast to mother-father or mother-alone. Then we should ask what the process is like that they go through to do that. And then we want to know what all that has to do with the child's behavior in the classroom. That would represent a precise small area to look at that would integrate those three domains and would be about as useful a model as we could generate for family research.

REISS: Another way of distinguishing families for study purposes is on the basis of the true hierarchical structure within the family. This leads to an interesting empirical issue: Under what conditions such a structure leads to a pathologic outcome and under what conditions it may lead to a very successful outcome.

MADANES: We need to include hierarchy as a variable in family studies. That is a common variable that runs across the three types of path-

ology with which the Institutes are concerned. That is, by the nature of the situation of having one disturbed person in the family, that person is in an especially weak and helpless position, yet at the same time that helplessness is a source of extreme power in relation to the other family members. The child derives an incredible power from the family's intense concern. Issues of hierarchy seem to be very relevant to pathology. It is the same with a drug- or alcohol-abusing partner in a marriage. This is because power is important. We organize in terms of power in all social organizations. In the family, the power allotment defines the hierarchical structure.

COLEMAN: Power and hierarchy are coping mechanisms. So what has happened in these families that leads to their choice of coping mechanisms? Can we look at what happens in a family, what kinds of changes, episodes, occur across time? Using David Reiss's three domains would be helpful—composition, process, underlying structure. We need to be able to look at alcoholic families, families with cancer, families with other kinds of problems—with illnesses, pain, and so on.

REISS: This suggests the dimension of history, which is another dimension that distinguishes families. The advantage of collaborative research, particularly on longitudinal samples, is the opportunity to improve our methods for grappling with the issue of history in given families. Otherwise there are enormous methodological problems. What is myth and what is reality? Sandy Coleman's data[1] about loss and death will lend itself to examination by a group like this—to see what it suggests in terms of theory, prevention, intervention.

McCUBBIN: How useful are models or typologies of families that are developed from clinical populations, which are atypical and extreme? Can they be applied in a prospective design and hold validity in the long run? We replicated or tried to test out those hypotheses using a life change inventory. And we're finding that it doesn't hold up. When you look at life changes—particularly in the loss area—and look at the adolescents that are non-heroin users in the normal population, you can't make a decent prediction between a pileup of changes in the loss area and the ultimate abuse of substances.

COLEMAN: It isn't simply what happens around death and loss. It's how the family *processes* what happens.

* * * *

McCUBBIN: It's sometimes very difficult to work with epidemiologists because when we talk family, we're not talking along the same lines. They are talking more about demographic, structural variables, whereas I'm interested in process variables, in the dynamics that go on within the context of the family which would shed light on how these variables operate together. But we really need to know a lot more about families from an epidemiologic point of view and from a process point of view, independent of any of the categories that presently exist—whether it is drugs, alcohol, or mental health.

Another question we would be especially interested in is—what are the mediating factors within family life? We can't just look at the family as a catalyst for pathology. The family plays different roles. In some cases it is a facilitator for pathology; it does develop dysfunctional arrangements. But there's something about the family that serves as a mediator, a facilitator of successful adjustment, of successful development. What are those factors that are not clinical concepts? What are the more positive features of family life that become visible targets for prevention? For example, what are the coping strategies parents use just to manage normal life events?

Outcomes

DURELL: If you take a systems view, one of the things that gets cloudy is what is the dependent variable. In a system you believe there are all kinds of possibilities for what you call recursive/non-recursive effects. You turn your research in the direction that your research or clinical question leads you. It's possible to consider the child's behavior as outcome and still maintain a systems view of the determinants of the outcome.

STANTON: Are we looking at family outcome? Or individual outcome? Or can we marry the two?

REISS: When the child is the focus, it limits the vision of the observer to a thin crust of the family. One looks for proximate variables. It could be contended that if one forces oneself as a researcher, or as one who intervenes, into thinking about a broader range of family outcomes simultaneously, one is simultaneously forced to look further into the center of the family itself, to find certain underlying structures of family functioning that are not necessarily proximate to any one individual but sort of lie at the center of all of them. One is then forced to discover aspects of family life that otherwise would not be seen by selection of a particular variable. Another issue that arises, then, is whether to give in to what one can be funded for in terms of research, or hold out in terms of our discussion of these for a little while longer, around the idea of what family level variables become dependent variables.

If we reduce delinquency in a family at the cost of or coincident with an increase in depression in the family, can we say that we have yielded—from a family perspective—a positive outcome?

KELLAM: If we're going to be interested in predictors, we need a profile of outcomes. They need to be specified. There need to be multiple outcomes—psychological, social, behavioral. That's the only way we're going to know how our predictors are acting in relation to outcomes. This is all part of the reason we need to get together. The Institutes have tended to be divided up, for political reasons, by outcomes rather than by concepts like family, or child, or adolescence.

DURELL: One area that brings us together and makes very real the possibility of collaboration is *adolescence*. One of the forces that brings

Institutes together in this endeavor is the recognition that when it comes to dealing with adolescence, having three Institutes functioning separately doesn't make much sense.

Mechanisms and Methodologies to Facilitate Collaboration

JACOB: Other issues in need of emphasis or study are the development and refinement of measures of interaction and process level analyses.

STANTON: What you are saying is that just to define present-day methodologies isn't enough. There should be a focus on new methodologies to examine processes that we have hints of or see before our eyes but don't yet know how to get at.

KELLAM: We need to integrate process variables—which have been part of family studies in relation to psychopathology for at least 2½ decades—across family types to see how the family types differ. We need to get the community epidemiologic level and the small sample levels together so that we can inform each other in our research design. We can give you a frame—a map of the neighborhood in terms of its social and familial structure—in which to do studies of process, and from which we can then look at outcomes of the children. That kind of research integration we've not been able to do because of our separateness. Something structurally has to change so that there is this integration. This is a policy matter.

REISS: Research jointly supported by the three Institutes that might facilitate mechanisms whereby many investigators could have access to carefully constructed samples would offer a genuine process to support family research and would permit an improvement of family research by allowing investigators to explore and compare results on samples that are otherwise very expensive to collect. A corollary of that is that there probably are not any investigators dealing with small samples who don't long for a major methodologic assist in how to make those small samples better. There are some kinds of studies that can't be done on large samples because the collection of so much data is required. Many of us are embarrassed about how unrepresentative some of our samples tend to be.

We need more collaboration between longitudinal researchers. They have to be highly selective and then stick with their initial variables, or else the outcome is meaningless. This restraint may be met by better collaboration among people who are spotting critical variables, and the cross-sectional researcher could make enormous contributions to longitudinal research because he'll purify the variables. Then you have deliberate dialogue. The job of the cross-sectional researcher is to figure out the observational setting that will elicit the critical variables. It's too expensive to include the wrong variables in a longitudinal study. We want to have the best variables possible.

VAZQUEZ: I want to comment more from the clinician's point of view. A lot of the work in family therapy is clinical work. The whole idea of working with families stems from trying to look for findings that get

away from asking people what they think or feel, as opposed to observational data. It depends on your system of explanation. A good deal of family therapy was developed in the first place by making the distinction between behavior by itself and behavior in context. The whole field developed and exists on the basis that putting things in context changes them, changes the meaning, changes what you see in front of you. And family process—or behavior of any kind—is constantly changing in relation to behavior between people. And there's a lot of difficulty in applying hard research principles to that sort of activity.

KELLAM: What he's saying is that there's only one way to look at the process variables in the family, and that is by observing the interaction. I'm saying that the epidemiologic approach often taps the feelings of people, asks them how they think or feel about an issue rather than observing an interaction. From my perspective, I don't know which of those is hypothetically more predictive of a child's behavior. What I'm suggesting is that there's no reason to believe that putting those two together wouldn't be informative for each group in a design for family research. There is no reason to assume that observing interaction is more valid as a predictor of a child's behavior than obtaining reports or feelings of people about the child or about themselves. The question is whether you need to interpret the methodological predilections of people from different perspectives. Science requires an integration of these methods.

VAZQUEZ: But your conclusion would then be tied to your methods.

* * * *

REISS: We need a common set of questions built on common sets of ideas about how families function. There has been a major increment with these sets of data. We have found this kind of work extremely valuable in our own research, and I just want to mark that as something that will make the methodologic disputes more manageable, because we'll have common questions. So the issue is: What are the mechanisms by which family process influences child outcomes? That's the basic question.

LANGNER: I'm arguing here the fact that for different types of outcome—dropouts, school problems, arrests—we could talk about some common set of measures in family—hierarchy, communications, and so on—and could probably cover most of the major problem areas that the Institutes are concerned with.

REISS: Tom Langner's data set[2] advances the field. It shows there is a connection of some kind between specific family types—whatever constraints on the definition—and specific child outcome. It says it on a very carefully collected sample.

One of the tables is an interesting model. It responds to some of the issues raised. There is an emerging implicit assumption in some of the charge to this group that there might be a non-specificity of family disorganization and outcome. This table is moderately to highly specific in terms of the connection between specific family structure and specific outcomes. It's a model table in the sense that it's the kind of data that one

would hope to collect from a variety of perspectives to answer this question. And we won't get too locked into whether it's the report of the matter or the direct observation of the family that's the issue. That's a sort of methodologic dispute of minor proportions compared with the basic question of how does family process influence outcome.

STEINGLASS: One of the things I'd like to see done with these measurements is to have them developmentally based.

KELLAM: An understudied area of tremendous importance is adolescent developmental tasks.

STANTON: You can't separate that from phases of family development, stages of the life of the family.

KELLAM: For example, demographic studies show that there is high mobility in the early years of childbearing and childrearing. There's the effort to get the kids out of the neighborhood and into a better location. So there's enormous pressure on the family from that perspective—to deal with the family qua family. We need to integrate that perspective with the interactional process—observing, getting people to express feeling. We also need to look at that piece of the larger family life cycle that seems to revolve around the teenager and is related to adolescent developmental tasks.

STANTON: School phobia is one of the clear examples of different ways of looking at a behavior. Earlier we thought the child was worried about mother and stayed home to protect mother. Then some looked at the mother's panic about having the child leave—giving messages such as "If you leave, I'm going to fall apart"—and said that she was at fault, holding onto the child. Or the father is away a lot and now the child goes, too, leaving mother even more alone. Each one is contributing; the position of each one is true. Yet the larger picture is the whole set of patterns of family interaction.

* * * *

STEINGLASS: To me the issue is that of researchers at risk, and the question of whether people in our field have the kind of backing to do what would be high risk studies for us, meaning we would be devoting 5 years to the same thing. So I can't divorce the question of subject matter from the issue of the political constraints within which funding works and the kinds of organizational shifts that would be necessary to support collaborative work that might subsidize a sort of risky shift. Perhaps a group of researchers could somehow hold hands and jump into the pot together. And through a variety of mechanisms—a number of which we've talked about, for example, just exchanging ideas—they could sort of sustain themselves through a troubling period. It's very important to know that somebody has gone into the field and emerged at the other end unscathed. Also important is the notion that there are other people out there. Then you don't turn around and find yourself out there alone. So I really do think that we have to simultaneously be talking about several points of view, not only from the point of specific topics but also from the perspec-

tive that there's a systemic process that has to go on to develop certain kinds of group structures in order to take the risks that I certainly would like to come out of this venture.

PART II:
FINAL SESSION AND RECOMMENDATIONS

STANTON: I'm going to propose three questions as guides for our discussion today:

1. If crosscutting research is considered important, what are some of the compelling reasons?
2. If crosscutting research is important, what are some of the requirements on the part of the family research community?
3. If crosscutting research is important, what are some of the requirements on the part of ADAMHA and the three Institutes to facilitate such research?

For a number of years some of us have been doing research that cuts across these various problem areas—drugs, alcohol, mental health. The notion of crosscutting research has not been that much of an issue for researchers.

VAZQUEZ: For us the notion of crosscutting research has been an issue. We're funded to do research on child abuse and substance abuse with families that have both problems, We are first of all a group of family therapists who got together and said, "We want to look at families." And being family therapists, we are interested in any kind of problem. We're interested in *families*. But we chose one set of problems basically because that's where the money was. We could just as well have done family research concerned with a number of other behaviors. The reason we picked this particular set of symptoms was that in one group there was one person who was a career teacher in alcoholism. There was also a pediatrician who was interested in work with child abuse. And there was myself, a family therapist. So the agencies approached us from their fields of interest, looking for therapists to do something. So I said, "Whatever you bring to me, I'll do it. I certainly don't screen out my clients, so I'm not going to screen out anybody else." And what was being offered, then, by way of funding, dictated what we were going to do, and not the other way around.

Then you have someone from one of the fields—an alcohol and drug abuse expert—who wants to make sure, for example, that you include in your instrument a certain number of measures that will give them the opportunity to compare with other projects that are geared specifically to substance abuse. The substance abuse people were appalled that the child abuse people could develop questionnaires and ask only two little questions about alcohol and let it go at that. They wanted a couple of pages. This problem is a very real one. What should you do? People in the substance abuse fields have spent a lot of years gathering knowledge. They believe substance abuse is a major problem, a very general thing that is spread throughout the population, something that needs to be researched very carefully. But people outside the drug and alcohol abuse fields don't pay so much attention to those problems.

Then we found the same thing with the child abuse people. And since we were basically a child abuse funded project, we had to pay attention to that. The child abuse people are as extensive in their requirements as the drug and alcohol abuse people. Then you get into such things as, "Well, child abuse and sexual abuse are two completely different things" or, "this kind" of child abuse. Then you get into "child abuse" and "child neglect." Are they the same? These considerations require very minute details multiplied to such a degree that they take over what you're doing. You end up doing nothing but dealing with detail. So clinically and researchwise it becomes very much an issue.

Clinically, the interesting thing is that as a family therapist one can talk about "behaviors," whatever they may be. But the alcohol people have problems with the notion that if you take care of the troubles in the family interactions, the alcohol problem will go away. It is not an acceptable notion to them. We do have to deal specifically with the symptom. Then you end up at a crossroads because a lot of the interventions have been developed, not from a systems point of view but from other points of view. How do you put them together without doing harm to your basic approach? We have spent many, many hours with our substance abuse consultants advising us, "But you people have got to mention AA more. Because nothing else works." And we say, "But wait a minute. That's up to the family." Or the family comes back from AA with some idea that goes contrary to what we're doing. Then what do we do?

So for us it's a very real issue. This is just touching substance abuse and child abuse. If you start dealing with other categories, such as psychosomatic illness, you get the same thing. You find a huge field ready to tell you what to do, both clinically and in research, but the question still remains: How do you integrate these things?

On top of that, from the family therapist's point of view it is hampering to have to think in terms of the symptom, up to a point. This is because we work with families, and families are families—period. In the project I mentioned it certainly would seem a lot better if we could look at families first, and deal with the categories of behavior after the fact, not before the fact.

KELLAM: The word "family" is too general. Thousands of people—researchers, interventioners—are interested in "family." So you have to ask: From what perspective? You can emphasize family as child-rearing agent, in which case the child is outcome. Or you can look at the family as social system, in which case you are interested in communication. I have trouble in our conversations knowing whether it's the structure and the requirement of research which is the problem, or whether it's the particular issue of funding institutions and the way they impinge on that research design, or what exactly the issue is.

STEINGLASS: I think there are problems in all those areas. Suppose you take a family that has a clinical problem and they're trying to get some help but they don't know exactly what they need. They don't feel good,

and something is bothering them. So they look around and try to get help. In our typical urban service system, they have to define themselves in some way. So they define themselves as a drug-abusing family and then they go to agency A. Or they might show up in a community mental health center and somebody would send them to agency A. In this area if you show up in a family service agency and they do an intake history and discover an alcohol problem in one of the adults, they will tell you, "You're in the wrong place. You belong down the road at the alcoholism treatment center." So this process of trying to match the feeling of disquiet that exists within the family against the definitions they have to meet to qualify for acceptance in the treatment agency is a process of the family system trying to integrate itself with the treatment system. And I hear you saying that some researchers have had that same experience in the design of studies. They've been interested in certain things that are not yet as sharply defined but give them the feeling that there is a payload in this particular area. Then the question is how to shape it, how to frame the question, the research question. And perhaps the organizational structure of the Institutes is exercising an influence on the shaping of these research questions that has been leading to premature closure in certain areas.

KELLAM: To what extent is it a problem or an advantage to look at alcohol, drug abuse, and mental health as outcomes together—integrated into family research—versus any one of those as single outcomes or, even more narrowly, any piece within one of those alone? This is a big question in regard to research. Institutions like NIDA, NIAAA, and NIMH do impinge on research. There's also the burden that to do research impinges upon political processes.

VAZQUEZ: Just as families get sent to the alcohol agencies, researchers also get sent money to do "this," and not anything else. For a clinician it's a problem because families don't just have alcohol problems. They have "family problems" like everybody else.

KELLAM: Now there are two questions on the floor: (1) How much does profiling, that is, integrating, interfere or help the research; and (2) what is the process of relating to institutions that give us money, and how does that process impinge on research?

The issue of the value of a profile of outcomes rather than a single outcome is the central issue for this meeting. NIAAA, NIDA, and NIMH are funding different outcomes. The result of that, I think, is a catastrophe for research. If you're looking at longitudinal predictions, plotting causal paths, leading from early material as it evolves toward these outcomes, you must have a profile of outcomes to know whether the antecedents are specific to any one outcome or whether they're general to a whole bunch of outcomes, and what categories of outcomes they predict. That's the only way you can make sensible causal models. So we require the Institutes to find a way to integrate their research funding. There is no other way to do sensible research. From my point of view, the absence of integration has been a serious hindrance to the kinds of research we have been doing.

McCUBBIN: What are some of those basic questions that have been bastardized to some degree to accommodate the split? We started out with broad research questions that called for a profile of outcomes, but those questions were eventually funneled to different areas. For example, our original interest was really looking at adolescent health behaviors that cover mental health, alcohol, and drug abuse. But we couldn't do that. There was no agency that would accommodate that study. Then we had to say, "Which agency seems most interested in it? Well, drug abuse is interested, so let's rewrite the proposal." So we moved away from adolescent health practices, which is our primary interest, just to accommodate funding. We have been doing crosscutting studies—yes—but at a tremendous expense.

When I write our reports I still want to look at adolescent health behaviors. And, yes, I do have the data in the bank because I slipped in the questions I really wanted to get at in the first place. But because the demand is to produce reports to answer the original research grant, that takes up all my time. In addition, the data become outdated, perishable, and you wonder if they're timely any more. So the price is really tremendous when we have to accommodate like that. I would still like to get at the basic research questions that we eventually had to compartmentalize in order to accommodate funding.

KELLAM: Take the drug abuse area. We're interested in looking at antecedents of substance abuse in teenagers. Now, if you want to know the antecedents of substance abuse in teenagers, you have to have more outcome variables than substance abuse. Indeed, substance abuse covers more than NIDA supports. It covers alcohol, it covers cigarettes. The fact is that even if you're interested in a narrow outcome, you need the alternative outcomes to see what the specificity is of your antecedents. So on both counts we need to have some more competent integration.

LANGNER: We do need these multiple outcomes before we can really begin to understand things. I certainly have no intention of looking at "broken homes," or "the broken family." Anyway, it's beyond my wildest dreams to work in terms of 1920s, 1930s problem research. But that's the way things are still being funded. So I'm trying to follow up 2,000 kids—and I have money for perhaps 300—to look at a problem that I really don't have that much interest in because I know that what I really want to study is the family. It's a terrible situation for family research. What can be done about this problem mechanically?

STANTON: I'd like to add here the question of the interchangeability of symptoms. I can't tell you how many families I've seen in research and in clinical practice where the youngster is a problem today in school, and when there's no change in the family, tomorrow it's drugs or something else, as long as there's a problem person. It may be the same child. Or, "He's not a problem as far as drugs, he just never leaves his room." So from that point of view—if you look only at the youngster's school problem, or if he turns to drugs—you would have to say, "Well, we can't look

at that, because we're funded by NIMH and that isn't included." So you're caught in a crazy bind. You do have to look at the whole picture of what's happening. And I'm hearing rather clearly that this is an issue for researchers, not only *qua* agency. It is also an issue for fields—to take the child abuse/substance abuse case—not only for agencies, but fields in general.

JACOB: I don't see the issue quite as other participants do. The major difficulty is in designing rigorous procedures with appropriate control groups and a range of outcomes to lead us to the most accurate conclusion. Often those are very expensive designs. So if we're talking about collecting a profile of outcomes, talking about comparing across problems, we're talking about something very expensive. Then when you come in with a budget of three to four hundred thousand dollars, in order to do this, the question is raised about what is most important and what would be most helpful in this state-of-the-field. In my work I've never been hampered by the difficulties some of you have talked about other than what it means in terms of the effort, what it means in terms of the cost, and the likely outcome of the proposal.

Another design alternative is one where you start with a definition of the family to include, for example, a psychopathological disorder—families with an alcoholic, or families with a drug abuser, families with a schizophrenic. When it comes to refining your theory of your model, you not only look at the contemporary structure of that family in comparing and contrasting that with other problem families, but you can also look at consequences of that family structure, both concurrently and subsequently. What is the impact on the development of the offspring given such and such family structure with such and such types of individuals? In the family research field there could be comparing and contrasting across groups with your experimental and control groups consisting of differences in the type of disturbance and the type of dysfunction and the type of maladaptive behavior that is represented. For example, families containing alcoholics versus nonalcoholics versus depressed versus schizophrenic.

KELLAM: In epidemiology there's a very different problem. For example, whether you include the profile of outcomes or not, you need the same number of cases with a single outcome in epidemiology as you would for multiple outcomes. Indeed the research design is dramatically strengthened by including a profile of outcomes. At that level of research you're going to interview the families, and you need x number of families to talk about causal models. It doesn't matter whether you've got a single outcome or multiple outcomes. In fact, it would be nice to have those cells filled. For the case control studies you probably would have to increase the cell numbers and cell sizes the more outcomes you look at, or the more comparison groups you study.

• • • •

STANTON: Where are the requirements on the part of the family research community?

STEINGLASS: We've pretty well covered that issue. There are impediments that are created for research in general by specialization in the Institutes, but I hear around this table a conviction that it's particularly true of family research. It has not just been very, very difficult, it has been more than that. It has been something that has genuinely retarded and inhibited the natural developments of the field more than might have been true in other areas. And that has something to do with the fact that the family as an entity of study is fragile enough, diffuse enough, heterogeneous enough that it tends to have difficulty standing up against the other ways of cutting up research areas. Alcohol is easier to define and use as a selection criterion than some descriptor of families or some typology of families that has been a little bit more difficult to see. Nevertheless, for a number of us in research there is the feeling that in terms of long-term payoff, the ability to move in the family direction will be more valuable than splitting these things up. For example, it seems to me that research organized around the family as a naturally occurring unit makes more sense than research organized around a peer group of people with alcohol problems. That has obvious political and social consequences, but for me the peer group is a less powerful and potentially less influential unit than the family, and I feel we can learn much more about how the family functions than about how the peer group of people with alcohol problems functions.

LANGNER: For the funding power structure the word "family" does not ring a bell. Cancer does, and heart disease, especially for people who are concerned with their own lives. But children are low priority and families are at the bottom. There's no real constituency there. Crime and delinquency—such things as when there is crime in the streets—all have their day. But families just don't make that much difference. The term is too vague. We should formulate a way of explaining what it is about families that we think is important. At least we could talk about families in relation to certain things that have important political overtones. If we said that families are important for the following shibboleths: delinquency, crime, drugs—then it would have some impact. Family doesn't carry any weight emotionally.

VAZQUEZ: It's not surprising that that's the way it is. If a lot of funding for research is based on rigor of measure, then for things like alcohol or behaviors that can be counted, it's very workable, and those measures have rigor. But for family, they say, "What do you mean 'family'?" They want to talk about ages and levels. And in trying to make that jump from things that can be easily described to the notion that there's something else going on in the matter of interaction between people, the guidelines as to what constitutes adequate or not adequate research aren't very relevant. We need guidelines for those concepts that aren't very clear.

LANGNER: The medical model has also influenced this. Some outcomes like depression and schizophrenia have their own centers because this is something solid. But people who are doing family therapy are using

a completely different model, different ideas, the holistic approach. And that certainly does not reflect the usual medical model.

• • • •

STANTON: For family research, what kinds of changes would we recommend for review procedures?

KELLAM: I think there's a need at a level above NIDA and NIAAA and NIMH for some center that would be set up to cut across areas. I'm not talking about research centers. I'm talking about a kind of center that could promote, enhance, and monitor integration. It would look at a problem area, pull the relevant people together, and might at times even get an ad hoc review group together to review an application.

STANTON: There has been an ongoing problem in "marrying" the child and family fields. For instance, we have facilities for marriage counseling only. These represent different levels of thinking about a family. You can focus at a level for an adolescent, or child, or the parental generation. But those levels, defined in that way, are limiting. Clinically, if you're working with a family, you may segment it at various times, deal with the children, parents, or with all of them together. If we're talking about family, I would be opposed to something that is so constraining as "child" or "adolescent," with "family" tacked on. I think that's a throwback, and that we're just beginning to emerge from that.

KELLAM: I think I'd disagree with that. I think the childrearing family, including the kids and teenagers, is a real family focus that deals with a stage of life.

STANTON: Of course it is. That is a subgroup within the larger family, and that family will come to later stages of development. But you could take it to the extreme, which would be to establish an institute for each stage of the life cycle.

KELLAM: I think the focus could be on either child or adolescent, presuming that family is seen as the primary social system in need of studying.

LEVIN: You can't assume that.

KELLAM: Okay, then let the focus be the childrearing family.

STANTON: That's the problem. Even if you are thinking about the childrearing family, your proposal would get shunted over to a unit concerned with adolescent development.

LEVIN: There's no receptor place in the ADAMHA structure for that, for the family as the primary social system that supports child or adolescent.

KELLAM: Then I guess I would say that the childrearing family is an integrator, that that's a way of looking at a focal point of social and behavioral and biologic integration around a domain. I think we're looking for integrators.

One of the things that strikes me is the independence of the people in epidemiology from the people who are doing research on intrafamilial process. It's not uniformly the case. But as a group we tend to be separated and do not appreciate the complementarity of epidemiological data and data on interfamilial process.

It is obvious to any epidemiologist that the people who do come to your clinics are not distinguishable from the people who do not come only by the symptoms that bring them. That is, the reasons people go from what we call first level—for community epidemiology, the total population—to an agency are not to be explained only by symptoms. In fact, symptoms may play almost no role. If they do play a role when they get severe enough, it certainly is not the only role. We don't know how many people with thought disorder or schizophrenia are in the community and do not come. The communication patterns you study in the second-level clinic, if they're not informed by a sampling from an epidemiologic perspective, are unlikely to be generalizable to a population described as schizophrenic or even to a population of families. These are not random samples of families that come to agencies. They are in fact very peculiar families. They're families with an interest in coming to that agency, which automatically differentiates them.

REISS: I don't think we can make the sponsoring Institutes responsible for dealing with large-scale social issues of the kind you have mentioned. And I think the issue on the floor really seems to have a much more practical intent. I think you're asking now whether this group recommends specific changes within cultural biases that agencies can't be responsible for counteracting or perhaps intelligently taking account of. For example, what kinds of review processes would be useful? I thought I heard you say that there was also a joint responsibility of the family research community to get its act together sufficiently and with sufficient clarity so that whatever case it wants to make about such changes can be made on the basis of the maximum amount of substance. I have some thoughts about that, and I'd like to hear the group stick as closely as possible to the kinds of things that might be done in the next year or two, given the kinds of built-in limitations in the way agencies work. What can we as a group either say now or talk about learning in the very immediate future that would buttress those recommendations and buttress them in the kind of language and support that agencies have the right to expect?

Let me just summarize a couple of points that have come up and specify that I think these are areas that could be strengthened in terms of the arguments of the family research community. I think the kinds of questions we've heard from agency staff, whatever our predilections are for analyzing these positions, have to do with issues such as the following: Is there a case to be made that families in some sense cause or contribute to serious pathologic outcomes in individuals? Is that causal system specific or general? And whether it's specific or general—that is, whether family factors cause many disorders without specific outcomes or specific family factors cause specific disorders—is there any evidence to believe that knowledge about the family factors will improve treatment or prevention?

Those are the questions I think are being asked of us as a group. And from my own perspective I think those are very legitimate questions for an agency to ask, although they may not use the language we would use in an-

alyzing clinical problems. But they are very legitimate questions for a government-funded agency to ask, and those questions may even represent a fairly radical departure from the kinds of thinking that goes on in the legislative supported agencies in which the issues may be much more closely geared to a social control question: How can we get rid of this and get rid of that and get rid of the next thing? The agency has really modified that, as I understand things, and has tried to change that kind of pure social control question into a more generous, humane kind of question. I think we as a research group have to answer the questions in the language in which they are posed as best we can. I feel to some extent it is incumbent on us as a group here to give some sense of what we feel about those issues and what the evidence is to support them. There have been studies mentioned about the family as precursor. I'd like to have people here give their sense of where the field is on that issue.

On the issue of review, I hate to suggest that the agency change its review procedures, for example, or that it set up a special study section on family process—which is, I think, to some extent where the discussion may be leading—without making it clear why such a section will really advantage their mission, not ours. I would like to hear people talk specifically about that point because I have a sense that if you set up a separate study section to fund projects, you have to attach funds to that study section. And there are not going to be increased funds coming from Congress to study family problems. In all likelihood those funds are going to be subtracted from neurobiology or epidemiology or child or something else. There will have to be a reallocation of resources within the agency. If we are asking agencies to do something differently, we've got to tell them very clearly why it should be different, with evidence they can use.

KELLAM: I was making a very concrete suggestion, which was not that we set up a study section on family process. That's exactly what we do not need. I am arguing that in fact we need a mechanism for bringing study sections from these three Institutes into closer working roles with each other around specific projects having to do with the family. I would want to see it focused on the childrearing family, not because that's the only way to do it, but because it is in keeping with the issue of prevention. The childrearing family is central to prevention, and indeed those of us who have done research in the field on the family and on child outcomes need some greater working integration.

There are two parts to my suggestion. One is a center, whose purpose would be to integrate a focus on the childrearing family across the three Institutes, which would allow us a profile of outcomes of equal and balanced interest. Secondly, it may be necessary to do ad hoc grant reviews which deliberately attempt to integrate epidemiologic, even demographic and intrafamilial process studies more adequately in the review process, in order to promote integration in the field.

REISS: I'm a little hesitant because I don't have enough of a feel for exactly how changes in the current administrative arrangements would ac-

tually operate. I'm more comfortable talking about functions from which I think the field might now benefit. My sense of the record that this group has sort of etched on the minds of the people who are listening is that the family field is in a transitional stage. It is not as well organized or as coherent, both in terms of relationships between investigators and in terms of working under simple paradigms with a set of relatively well understood methods as, for example, neurobiology.

KELLAM: I feel the need as a researcher for a level of intercommunication among staff, which is a staff function, as well as a possible cost-sharing function, that cuts across NIDA, NIMH, and NIAAA. It's very important to recognize that there are two basic functions: one is a staff function and the other a review function. They're really two different problems, and there is a need for the staff here in these three Institutes to intercommunicate through some kind of mechanism.

LANGNER: Are the people here adequately representative of people who have studied families? We have the holistic people; then, epidemiologists who are pulling people and families apart. What kinds of people would be represented on a review committee? It seems to me that is a critical question. There are some committees that seem to be heavily loaded in one direction, and compositions have changed drastically over the last few years.

VAZQUEZ: That's very important. David Reiss mentioned the issue of different paradigms, and that certainly translates to different people in review committees having different paradigms.

LANGNER: That's not a problem. The only problem is sufficient representation. Review committees just have to be representative, they don't have to be in full agreement.

KELLAM: We're talking as if there is a problem. I'm thinking specifically, for example, about delinquency on the one hand and drug abuse, not alcohol, on the other, and that's the way NIMH, NIDA, and NIAAA are split. Now I am trying to focus attention across the three Institutes. Can you in fact bring these fields closer together? We would have to do that, for example, with delinquency and drug abuse, which have some relevance to each other, as well as to psychiatric outcomes, which is also an important consideration. So there could be some cutting across at the staff function side, and sizing up the field, encouraging applications, interacting with investigators, perhaps getting some of them together on a study.

STANTON: I think there are differences in methodology and paradigms, and the question is whether there are problems with peer review in the family area? Is there equitable and fair review?

REISS: I think it might be useful to try to define who the research community is that we are talking about. Who are the people, what is their training, and where are they getting their ideas? As a way of getting that discussion going, let me try to paint in several broad strokes what we are talking about. As I have seen the field really develop in about 18 or 19 years of my own work in it, there are really three parts: There are people

interested in the family who have come down from macro systems in sociology and epidemiology and tend to have very high levels of training in the more macro discipline from which they have "descended." Secondly, there are psychologists, some of them with a prime interest in child development, but others interested in other areas of psychology, who have in a sense "ascended" from psychology into the family field. The training of those people tends to be very strong in the various subdisciplines of psychology. To some extent each of those groups has been touched by a third group, which has been a mixed group of clinical psychologists, psychiatrists, social workers, and other disciplines, whose direct interest in families started with family therapy, and whose observations about certain peculiarities of families, certain arresting paradoxes in families, intrigued them. And they went one way or another, either to make use of disciplinary training or to pick up training along the way to convert themselves in part or in whole to family researchers. Their observations and their concerns have tended, in a number of interesting interdisciplinary exercises, to inform and shape some of the people coming down and the people going up from very orthodox and, to some extent, academically oriented or research-oriented training programs.

The kinds of issues we are talking about have begun to define a field. But because it comes from so many different disciplinary backgrounds, it is hardly a uniform one. And the kinds of issues some people have talked about repeatedly in this meeting—the notion of the function the symptom serves to keep a family going in a particular way, the notion that if you intervene and cure a symptom in one person you may very well get some kind of symptom in another person in that family—these kinds of seemingly paradoxical observations have fueled an interest in the integration among individuals within a family, but have also tended to pull people from various disciplines closer together. That's the way I see the field organized, and I think any policy has to keep track of the fact that that is how it seems to have originated. From a policy perspective, it would be interesting for the sponsoring institutions to get a clearer perspective as to whether my image of the field is an accurate one. It would be very useful for the three Institutes to have a sort of prehensile, firm, self-confident grasp on how the field is taking shape, who is in it, what are the training programs, and what are the means of support in the particular university and non-university centers in which this kind of thing is going on. If that kind of knowledge was held by a single group, issues such as who might serve on study sections, who might be appropriate ad hoc reviewers, who might do this or do that, would be answered with a more authoritative grasp. Because epidemiology, sociology, clinical psychology, family therapy—which out in the world are still very disparate disciplines—have come together around a set of paradoxical observations, it's relatively difficult in my view to keep track of where the field is. But I don't think it would take a lot of energy or money or initiative for that to be one component of a tri-Institute initiative at this point to clarify the field.

LANGNER: The anthropology field focuses on the family as the primary unit of study, and there is tremendous literature just in that one field. I can't think of a group on families that shouldn't include some anthropologists as reviewers. I don't think it's such an easy task to say what the field is. The literature is vast. And family research is funded in many different ways. NICHD[3] funds a tremendous lot of family research, even cuts it up according to ages of people. While here we are talking about whether we just stick with the child and the family, they run the gamut of the lifespan.

REISS: I think it's important that we not treat the field as a bibliographic index would treat it—that is, any article with the word "family" in its title gets included as the kind of research this group has tended to gravitate toward. What I'm trying to specify is that there is a cohering group of people, but not coherent. It's very much of a process, heading in a more coherent rather than a less coherent direction. We are organized around a set of paradoxical observations of families; we tend to relate to pathology of various kinds, to relate to these symptom rotation issues we talked about in connection with the family's specific role as a precursor in psychopathology, to relate to functions of symptoms for families. Those are common threads that unite many of these people, but it's by no means equivalent to just family, per se.

McCUBBIN: Even though the agencies may be funding different "family projects," I don't have a sense of coherence as to what is the general commitment here to family research. What questions are being answered by the different agencies that, when we pull them together and really look at them, are really building on some piece of knowledge or improving the field? Maybe that is not the purpose of the various agencies here, but I don't sense a unified theme, if you will, from which to pose major questions that I think the field would like to answer.

KELLAM: I like both these comments. Having worked part of my life in this institution and the last 17 or 18 years out in the field, I don't know that we are aware of the enormous problem that a structure imposes on the research that we do and on the communities of researchers. When the three Institutes were set up, I don't know that anybody could predict how further splintered that would make the scientific community. To put delinquency in NIMH and drug abuse in NIDA and alcohol abuse in a third place has enormous impact on communication patterns and on the integration of scientific knowledge. The feeling in the field is one of increasing fractionation. The advances in knowledge have been dramatic in the last 15 years. If you just take the question of early predictors of later adolescent outcome, there is now a fairly substantial body of predictor studies which show a fairly high level of consistency. Unfortunately, it will take a deliberate act of social structure to pull the people together who have done the predictor studies and get them to talk to each other, because there is no centralizing, integrating function now. We need to deliberately set the next stage of our work, involving some mechanisms to cut across NIDA,

NIAAA, and NIMH around a particular focus. I think that there are a set of such focal areas, and I don't mind the fact that some of them are even political, like the childrearing family.

STANTON: For the most part, the review process as I've experienced it does not include many people who understand the paradigm pertaining to the kind of group David Reiss has described—that middle group coalescing around these paradoxical processes. I have not experienced in the review process any people who understand the kinds of processes we are talking about. Now I may be wrong in being all encompassing, but I label this a major problem, and I've seen very credible research gutted because the reviewers didn't know what the investigators were talking about.

As an example, let's say a problem of any sort breaks out in a family and has a systemic basis to it. And to go back to the crosscutting issue they could say, "Well, if it's drugs..." But suppose we don't know what it's going to be. We make an intervention and watch for what change occurs. It could be this or this or this problem. "Well, if it's going to be that problem..." But we don't know. It's sort of a prospective response to an intervention—and then you're into crosscutting considerations and you don't know where to go. That's one kind of casualty that can arise from the process that now exists.

REISS: Do you feel that there are highly trained researchers, all prepared with credible designs to study a problem like that, who will not get funded by virtue of the present institutional structure?

STANTON: I know of one example, with another design, where there just wasn't understanding on the part of the reviewers. While the investigators were thinking in terms of a systemic paradoxical process, the reviewers broke everything down reductionistically to individual variables. And, therefore, the whole idea of a system was lost.

STEINGLASS: I'd look at it somewhat differently. What is a reasonable balance, for example, between hypothesis-generating and hypothesis-testing studies? And how does a review committee arrive at a decision about whether a field would be best served at a particular point in development by emphasizing hypothesis-generating, or whether it's ready for hypothesis-testing studies? It seems to me that that's the kind of issue that would require a more intimate knowledge of the develpmental stage of a field itself, and that has probably been missing in most of the review committees I've had experience with.

* * * *

STANTON: What methodological issues are of special concern to this group?

McCUBBIN: First of all, I don't think the field is in so much of an infant state. When you look at Straus and Brown's[4] compilation of all the measurement techniques, it doesn't reveal a field that is perfect, but it does provide us a landmark perspective—that there are certain measures out there that are being explored and tested even further, even to the point of norms being developed. But I think that kind of information needs to be

shared much more widely. We can't wait for some kind of handbook to come out every 7 to 10 years, and it's usually outdated by the time it arrives anyway. But I think measurement has progressed reasonably well, and that as researchers and review panels look at it, they will be quite surprised as well as pleased. The other point, which Tom Langner has already made, is in support of a much stronger move toward multimethods, particularly when we are looking at family dynamics where there is a need for an interaction between the clinical methodologies and stronger emphasis on real dynamics.

STEINGLASS: We've mentioned longitudinal designs, prospective designs, multimethod designs. Are there alternatives to longitudinal designs that get at prospective issues? Are there any alternatives, in looking at etiology, to very expensive and very long-term longitudinal designs? Are there specific transitional issues or specific crises or specific natural experiments that would be amenable to systematic study—particularly in being able to get pre-crisis or pre-transition assessments of family behavior—or are we also going to come in after the fact, not being able to tease out the question of whether it was the event or the family or some interaction between them?

I would like to see development of some reliable family assessment measures that are developmentally based. In other words, tying them to family life cycle issues. I am interested in naturalistic techniques that translate systems theoretical principles into measurable behavior, and in techniques that can develop family level variables that somehow tap into things like family temperament or family personality.

VAZQUEZ: From the therapist's observations and actual interventions came the study of process. I have my own predilection for studying clinical situations.

KELLAM: The first 30 years of the whole research enterprise has been enormously successful. Thirty years ago there was very little research, very few researchers, and very little knowledge. Now we have the sense that we haven't learned a lot but have accumulated enormous amounts of data in lots of small fractionated packets. So it's appropriate for us to take stock of both the structure and the research. We also have to deal with the fact that in the process of gathering and doing research, fractionation has been the order of the day. We now see three Institutes where there was one. We see research review split off from staff work. Those are very powerful influences on researchers and on the field, and all of that is important as background for our discussions here. It happens that that's counterproductive at the particular stage we're at for integrating knowledge, because the fractionations tend to make integration of information more difficult, not less. And it makes us deliberately attempt to figure out ways of communicating better at a time when we really should be communicating better, because the knowledge we've gathered needs putting together around issues of prevention and around issues of better treatment. The community expects us to be accountable for those things.

There are several levels of research on family that need more involvement with each other. One is the macro-demographic family study. Wendy Baldwin's[5] group is prototypical, as is Paul Glick's[6] work for demographers on stages of family. The community epidemiologic level is just beginning to be understood on an intermediate level. It means studying systematically the variations within a community, holding constant the gross characteristics of the community and looking at variations in the conditions—family and otherwise—that influence outcomes, whether pathology or school achievement or other kinds of outcomes. That level has been difficult for NIMH to generate. Epidemiology has had a peculiar role here. Unlike in Britain and other scientifically oriented Western industrialized countries, epidemiology here has seemed to be poorly understood. For many people, epidemiology is demography. It's high school student drug use for NIDA—as if Woodlawn and Winnetka and Montgomery County are all the same and we should generalize about them. Well, epidemiologists don't like to do that. So that level of community epidemiology is important. Knowledgewise something follows from that level, and that is that in a situation where there are many alternatives to mother-father families, it's extremely important that this level of work—that is, community study of variations in family—impinge upon the categories the demographers and census takers use to count families.

It's also vitally important for the intrafamilial, interaction studies to take into account the particular kind of family they're talking about. "Mother-father" is no longer sufficient, and "broken" is even less sufficient. We know that there are varieties of kinds of families. Mother-grandmother families are terribly important kinds of families in our community. We don't know how role differentiation in childrearing differs across family types, and we don't know how the impact of these differences relates to child and family outcomes.

So the community epidemiologic message about family research would be: Look at the variation in family types and study within those categories and compare across those categories so we can learn more about the alternatives to mother-father. We live in a society in which alternatives to mother-father families are terribly important. There may be gross ethnic differences. We find gross differences in a ghetto community between mother-alone and mother-father, and no differences between mother-grandmother and mother-father. Mother-grandmother appears to be a fairly effective family, at least in our community. We need replication and we need expansion of that kind of information.

This community epidemiology level needs integrating with the laboratory level in both the area of family and the area of child development. And in the area of prevention we can build on the kind of data bases we have, both in terms of specifying better variations of family structure and looking at the predictor studies across fields, which means across Institutes, unfortunately. We have literature in delinquency, which has some relevance to prevention. That is, what are the early antecedents of delin-

quent behavior? We're also talking about predictors of substance abuse, which has now a considerable body of literature. There has to be integration across those longitudinal prospective data bases, first by looking at the data now in hand and the analyses now in hand and, secondly, by asking if we can replicate some of these analyses across the different data bases that now exist and do some thinking about where the data are needed for further work. Can we do some intervention experiments which test the function of these antecedents on the outcome? Early aggressiveness, for example, we can push back to first grade and say that it has a very powerful relationship to substance abuse later on, and to delinquency later on. If you can change it in first grade, will you then alter those outcomes? Animal analog studies can help study breeding versus genetic transmission in aggressiveness to give us some hints of the nature-nurture kinds of issues.

This kind of integration can be brought about under a set of umbrellas like child and family—or child/adolescent and family—or even like prevention, where we take advantage of the mandate to show how to get to some better state of health with all the research money that's been given out. A little bit of social structure building, which should not replace the independent investigator but enhance our mechanisms and cut across these Institutes, will influence the researchers and pull them together for better communication. Some simple mechansims make enormous differences. For example, some large studies have asked for progress reports every 6 months instead of once a year, and these are then sent to all the investigators who have a grant in that field. That updates progress reports by about 500 percent and also improves communication. Wendy Baldwin then pulled everybody together once a year to talk to each other. It was an inexpensive, simple mechanism for pulling people together in this particular domain. We need these kinds of innovations between the Institutes.

• • • •

COLEMAN: We need some kind of family measurement, something like a battery, where you could take a cross section of observations in some systematic way, at various points in time—or in the context of a short period of time—to let us really take a look at what is going on within the family. We could get at the three main domains David Reiss mentioned: composition, structure, process. We need all of them. So somehow we, as a field, need to come up with a method, a design for getting at that, so that we can move on from where we are at the present time. In tracking the family patterns that I am doing, I have no way of going back and reconstructing what really went on. All I can say is that this is what was reported. Somehow, if we could get the total tracking of the life cycle, which we're doing, along with some measurements of the actual interaction—which is what Jaime Vazquez is so interested in—we'd really have a contribution to make to the Institutes which are allowing us to sit here and brainstorm. So we need to come up with some kind of measuring design. How do you measure the development of the family? How do you check it

out? These are things that Peter Steinglass alluded to yesterday. How do you know whether the repetitive behavioral sequences that we observe here are going to be observed later on, or were observed previously? Are the power hierarchies the same at this point in development as they were last time?

REISS: Picking up from Ham McCubbin's optimism on the state of methods, and to record in this group my own view that there are a number of very exciting, well-developed methods available for direct study of family process of the kind we are talking about, let me just list a few that I think are the most promising.

There are a group of methods which could be called "standardized assessment of family functioning" where whole families are brought into standard test situations and their response to the situation can be measured with a great deal of precision. These are procedures which have been used on hundreds of families in a number of institutional settings, several of them cross-cultural. I mention just a few of them to give a sense of the considerable progress that has been made.

The most used of this group of methods is SIMFAM, as Ham has mentioned, a procedure developed by Murray Straus when he was still in Minnesota,[7] which essentially puts families in a highly standardized stress situation and measures their response by objective behavioral criteria. For about 18 years, our own laboratory has been using a set of problem-solving procedures tested on hundreds of families. It's a procedure like Straus' SIMFAM which is culture free. The Consensus Rorschach has been used in a number of clinical settings and is a very evocative procedure. There is the extremely innovative work of John Gottman[8] on monitoring sequential processes in family interaction in both affective and non-affective spheres. The volume summarizing his work gives a good index of how far some of the technical aspects of direct measurement of family process have come.

One branch, somewhat less developed, is the re-creation through standard historical documentation of changes across generations in single families. Families are unique groups because of the impact of very remote events on current family functioning. We have an example in Sandy Coleman's work on the effect of deaths in previous generations. Themes across generations persist in remarkable ways. Continuity of major themes across generations in family life is something family clinicians know well. A blossoming field within the family area is the field of the history of the family. The value of this work to some of the Institutes' main missions is very important.

A third area is the area of home and neighborhood observations, *in situ* observations of families in their natural settings. We are now developing in the field quantitative measures for assessments of families in their own settings. We can learn about families in different contexts other than our own research laboratories, in ways that conform to very general standard criteria.

A fourth area is the use of animal studies and certain biologic tools for the understanding of family process.

LANGNER: I'm interested in process variables as opposed to the very popular life events checklist. Much of family research has been focused on the "broken home," "divorce," and so forth. When we do a regression analysis, putting in a lot of the things taking place in the family, by report or sometimes on the basis of observation, we find that if we add a subsequent set of events reported by the mother or the child—a life events checklist—we don't get any increase in the amount of variance predicted. This is very bad for the life events people. We are probably moving more toward the kind of holistic things many family therapy people are doing. One of the things we find is that very few events take place in low socioeconomic groups. There are very few events as you go down the social scale, other than terminal events. People don't get jobs. They don't get promotions. If you make a list of events, there are a lot of things that don't happen. And a lot of these things have to do with expectations and fulfillment. The whole concept of events is very important. We could say, "Well, this is so elementary that we know all about it." But a huge amount of money is being spent in this area, and I think we should have something to say about it here.

I would be very concerned with cohort effects. In our study of a 5-year period there were sharp changes. There were changes in parental practices, and there were changes in child behavior. I can imagine, then, what changes are going on in 10-20-30-year periods. So we should look at historical changes and try to isolate them from family changes that are developmental.

We ought to talk briefly about path analysis as a method. The two major social factors that seem to pack a wallop are social class and ethnic background. These are major facts of American life. In using path analysis we found that you can, in a sense, psychologize social class. It doesn't explain it away. It just helps to show that, instead of going from social class to parental practices—with the transmission across generations of some fairly fixed practices—it seems that poor parental practices are mediated by poor marriage. Maybe it takes years and millions of dollars for a person like myself to substantiate this when one can see it in practice. However, the effect is very striking and very significant. Poor parental practices spring up when there is a poor marriage. So I think that path analysis allows one to follow through time what is happening in the family. I am particularly interested in spontaneous changes as opposed to interventions. There are major changes in families over time. These changes have almost no effect on children in minorities. This is a very startling finding, in contrast to some thinking that children weren't severely or permanently damaged but were just getting negative reinforcement from their environment. The major problem is how to measure this change in families, how we go from one generation to the other. How do you measure

out? These are things that Peter Steinglass alluded to yesterday. How do you know whether the repetitive behavioral sequences that we observe here are going to be observed later on, or were observed previously? Are the power hierarchies the same at this point in development as they were last time?

REISS: Picking up from Ham McCubbin's optimism on the state of methods, and to record in this group my own view that there are a number of very exciting, well-developed methods available for direct study of family process of the kind we are talking about, let me just list a few that I think are the most promising.

There are a group of methods which could be called "standardized assessment of family functioning" where whole families are brought into standard test situations and their response to the situation can be measured with a great deal of precision. These are procedures which have been used on hundreds of families in a number of institutional settings, several of them cross-cultural. I mention just a few of them to give a sense of the considerable progress that has been made.

The most used of this group of methods is SIMFAM, as Ham has mentioned, a procedure developed by Murray Straus when he was still in Minnesota,[7] which essentially puts families in a highly standardized stress situation and measures their response by objective behavioral criteria. For about 18 years, our own laboratory has been using a set of problem-solving procedures tested on hundreds of families. It's a procedure like Straus' SIMFAM which is culture free. The Consensus Rorschach has been used in a number of clinical settings and is a very evocative procedure. There is the extremely innovative work of John Gottman[8] on monitoring sequential processes in family interaction in both affective and non-affective spheres. The volume summarizing his work gives a good index of how far some of the technical aspects of direct measurement of family process have come.

One branch, somewhat less developed, is the re-creation through standard historical documentation of changes across generations in single families. Families are unique groups because of the impact of very remote events on current family functioning. We have an example in Sandy Coleman's work on the effect of deaths in previous generations. Themes across generations persist in remarkable ways. Continuity of major themes across generations in family life is something family clinicians know well. A blossoming field within the family area is the field of the history of the family. The value of this work to some of the Institutes' main missions is very important.

A third area is the area of home and neighborhood observations, *in situ* observations of families in their natural settings. We are now developing in the field quantitative measures for assessments of families in their own settings. We can learn about families in different contexts other than our own research laboratories, in ways that conform to very general standard criteria.

A fourth area is the use of animal studies and certain biologic tools for the understanding of family process.

LANGNER: I'm interested in process variables as opposed to the very popular life events checklist. Much of family research has been focused on the "broken home," "divorce," and so forth. When we do a regression analysis, putting in a lot of the things taking place in the family, by report or sometimes on the basis of observation, we find that if we add a subsequent set of events reported by the mother or the child—a life events checklist—we don't get any increase in the amount of variance predicted. This is very bad for the life events people. We are probably moving more toward the kind of holistic things many family therapy people are doing. One of the things we find is that very few events take place in low socioeconomic groups. There are very few events as you go down the social scale, other than terminal events. People don't get jobs. They don't get promotions. If you make a list of events, there are a lot of things that don't happen. And a lot of these things have to do with expectations and fulfillment. The whole concept of events is very important. We could say, "Well, this is so elementary that we know all about it." But a huge amount of money is being spent in this area, and I think we should have something to say about it here.

I would be very concerned with cohort effects. In our study of a 5-year period there were sharp changes. There were changes in parental practices, and there were changes in child behavior. I can imagine, then, what changes are going on in 10-20-30-year periods. So we should look at historical changes and try to isolate them from family changes that are developmental.

We ought to talk briefly about path analysis as a method. The two major social factors that seem to pack a wallop are social class and ethnic background. These are major facts of American life. In using path analysis we found that you can, in a sense, psychologize social class. It doesn't explain it away. It just helps to show that, instead of going from social class to parental practices—with the transmission across generations of some fairly fixed practices—it seems that poor parental practices are mediated by poor marriage. Maybe it takes years and millions of dollars for a person like myself to substantiate this when one can see it in practice. However, the effect is very striking and very significant. Poor parental practices spring up when there is a poor marriage. So I think that path analysis allows one to follow through time what is happening in the family. I am particularly interested in spontaneous changes as opposed to interventions. There are major changes in families over time. These changes have almost no effect on children in minorities. This is a very startling finding, in contrast to some thinking that children weren't severely or permanently damaged but were just getting negative reinforcement from their environment. The major problem is how to measure this change in families, how we go from one generation to the other. How do you measure

when the child is taking over the parental role? One method I have used is that of regressed change analysis.

JACOB: I'm concerned about the possibility of sampling within each of the major family types described by Shep Kellam in terms of the interactions, interfamilial data. The issue of sampling within those types becomes a problem. How many types are we going to begin sampling? What would smaller samples look like numerically? A lot of the interaction variables would be importantly affected by other parameters. One has to look for some additional controls across these types—number of children and ages of children. It's not easy to get that small sample.

There are a number of complexities statistically in dealing with triads instead of dyads. The sequential analytic procedures can describe certain dyadic interchanges, but the complexity introduced with a third element is astounding in terms of the amount of data you need to begin analyzing those sequences. All of this has meaning for the interaction people.

I also see a need for a kind of mini-impact studies. We have some interesting measures and interesting variables, and I think further gains can be made by separating some of those and doing some further refinements. The example that comes to mind is discussed as "communication deviance" and its interaction with "expressed emotion" in terms of high predictability to clinic outcome. It would be of some interest to look at the impact of some of those verbal interactions on subequent behavior. There seems to be an association between the emission of some of those behaviors and the occurrence of certain very complex behaviors. And what we don't know is anything about the actual process by which that communication affects, potentiates, or alters behavior.

When we talk about levels of family influence, I can't dismiss the family genetic level as one which we could try to incorporate into designs that may come out. For example, what about the importance of genetic variables in the development or potentiation of alcoholism?

MADANES: I would like to see more studies of the types that are hypothesis generating. And I would like to see a general focus on issues of hierarchy and change.

STANTON: I would like to add—for intrafamilial research—that triads be considered. Dyads are very limiting, and there are very few familial systems that are two-person systems. We might also look at ongoing repetitive patterns and perhaps a typology of sequences.

KELLAM: We need to consider something more permanent in the way of structure in order to promote integration of family research across the three Institutes. What we want to do ultimately is to integrate the knowledge out in the field, from data which are not collected in a fractionating way from other data, and which promote the important theory building that necessarily will cut across small domains of scientific mode—little groups like family process groups, like epidemiology groups, and then there is this group of us.

We need to somehow get beyond this first 30 years of mental health scientific effort. Now to do that you need a very clear social structural statement that that is of value. And it matters to the folks in towns like Chicago what the structure is here in Washington. So if you make three Institutes, you're saying fractionation is okay. If you make one transcendent office that cuts across the three Institutes—with a command for studying, integrating, pulling together and fostering research integration around the area of childrearing family, for example, or around the area of prevention– you've made another kind of statement. I would promote the latter. I think we have not had a structure for the three Institutes and the Office of the Administrator that clearly commends integration as a value and assesses fields in the ways we've been talking about, promotes the advance of work in those cross-problem domains, and then looks around to see what needs to happen in review.

DURELL: We can't ask this group to do more than state the problem and make a general statement as to what kinds of things would be helpful.

STEINGLASS: I've been speculating about preconceptions that people had coming into the meeting, some of the exchanges that have gone on, some of the critical turning points in terms of the kinds of things that have been useful to the group in keeping the process going. As I've been tracking it, I have a couple of very important impressions. The first impression is that as a researcher involved in all of this, I am coming out of the meeting feeling that my field is in much better shape than I thought it was, and that is very encouraging to me. In other words, it may be that people in the field are the worst critics or the harshest critics of their own work. I am feeling some buoyancy about the state of the field.

Second, after and through some interesting mazes, twists, and turns, it does appear to me that a group of people all interested in the family, but with very different ideas about what that means and very different ways of approaching it, has been able to exchange ideas profitably, clearly not convincing each other of central points, but profiting from the exchange. In terms of the process itself, it seems to me that there have been some things that I call initiatives and some things I call a kind of maintenance function.

Let's take the second one first. What I mean by that is that there have been critical interventions or roles performed by individuals within the group that have served to provide sustenance and support to the on-going process, to keep us on track, and to keep things going in some reasonable direction. The initiatives approach has been from people who have pointed out major discrepancies or major problems, or have provided provocative findings or observations that have demanded attention, and have stimulated and perked up people at that point in time.

The maintenance function, which I consider to be absolutely central, has left me at the end of the second day with a sense of optimism. I believe that David Reiss has provided a very critical role in the group process as I have perceived it in playing that particular role. The question, then, is

what has he done? And to what extent might the kind of functions he carried out over the past two days be institutionalized in some way? Is there a need for that kind of person?

What he has done, as I see it, is two things. First of all, he's clearly been an advocate for families and family research. He has been the one person who has consistently said things are good, there are good things here, let's look at these interesting ideas. The second thing that was amply demonstrated this morning was the ability to provide a perspective. Where does this idea fit into some overall scheme?

Now I don't know whether such a person exists in the Institutes or in the ADAMHA structure, or whether you would find such a person useful or profitable. From my point of view, if I were in your position trying to make these decisions, I would consider that kind of person absolutely essential. So whether you are an executive secretary trying to make some decision about where a particular proposal fits in the overall scheme of things and whether you even have the people who are qualified to understand it in terms of the field in which it fits, or whether you are a program person trying to get some sense of whether the review committee has applied an appropriate priority score to the research that is understood by the people who are doing that kind of work, or whether you are somebody who is trying to decide whether there really are areas that need to be coalesced or are ready to be brought together or other areas that need to be stimulated—if your difficulties parallel what has gone on in the two days here—it would be very helpful to have somebody who provides that kind of integrative function.

I am in agreement with the issue of pulling things together, although I wasn't necessarily when we started off. That's been one of the very exciting parts of the meeting for me. I am not of the opinion now, having listened to the conversation, that this is a field that needs to get some central stimulus to encourage people to do research in this area. It sounds as if there are a million people who want to do it, and the problem is that they are complaining that nobody is listening to them or understanding what it is they want to do or perceiving their priorities as they would perceive them. It's not like the alcoholism field was a decade ago when no one wanted to do any research so you kept on putting out these one-shot RFP's* to try to get somebody good to respond to this area and to that area. In my opinion, it would be a dreadful mistake to move again in the direction of this RFP and that RFP that would have no continuity. We would again be in for 3-year shots to entice people to come into the field. The issue now, as we have articulated it, is integration across Institutes, but also integration within the field. The family advocate is the minimal structural thing that I would see as critical, both intra-Institute and across Institutes.

---

*Request for Proposal.

McCUBBIN: I want to emphasize not so much the political issue as the need for us to get together. It's a great value for us to talk and communicate. I also want to emphasize that there is a real scientific value for cross-linkages across the agencies. Let me comment on a couple of things that have been stressed in the context of this discussion. There are burning questions that family researchers in the field would love to tackle, particularly in the health and mental health fields and across the two, whether it be substance abuse or promoting good health practices in the context of the family. They would like to see these areas examined, but are not clear about the context in which such proposals would be pushed forward, or whether there is legitimation for it, or whether such an agency is even interested in promoting well-being besides just the understanding of pathology. I raise this because I am not so sure that by finding correlates of pathology you have answered the question of what's good for promoting good family health practices. I would like to see both ideas operating together, or for the paradigm at least to be ventured, as we begin to look at research proposals. I think there are many projects in the field—and I can speak from Minnesota and the Family Study Center, where we examine a lot of proposals—which we can pretty much predict from experience won't fly with the review process. They are lost in the maze of review, and people are not even sure they are in the proper agencies.

One of the most exciting things I was hoping for from this particular meeting was that we would have a forum, a context in which to talk about "good research questions." There are scientists in the field approaching research fully from a family perspective, examining the family as mediator, and the family as a crucial intervening variable for a lot of individual outcomes, whether it be pathology or health. I would hope for a collaborative effort across agencies where this message is communicated: "Here is the forum. We're interested. You come up with good questions. You come up with good methodologies, and we will be willing to look at them."

STEINGLASS: The issue about the review process that was brought up is a very important one to underscore in terms of the changing nature of the priority scores, the separation of the reviewing program, and the lack of flexibility that program people have to make decisions within priority order. The review committees that I have had experience with have varied tremendously as to the composition of people and their ability to judge the priority granting appropriately within fields. The issue of scientific merit often can be brought to some uniform conclusion by a "savvy" chairman, but then when people are asked to write down what they think is the priority order, they are often at a loss, and committees have adopted widely varying rules for that. One committee might ask for some guidance from the main reviewers about what they should write down for a priority score. Then you have the kind of business where there really are three reviewers who are attaching the priority score and influencing the other committee members who have come to trust them within their disciplines but feel totally at a loss as to where to place this particular program in its field. In

others, it is very much a private decision in which that kind of request would be totally out of place.

KELLAM: We have enormously important and exciting realities. One is that we have produced in the last 30 years a very large assemblage of very different kinds of investigators, and those investigators have produced an enormous amount of data in the form of reports and scientific articles. The review process in our social structure here and in support of that research enterprise was designed roughly 30 years ago. In the early stage there were few investigators and very little, comfortably speaking, research data. Now we have a very different enterprise. We have very large numbers of researchers who, by virtue of the nature of our society and of the enterprise, have increasingly fractionated into very small homogeneous groups for purposes of communication, reinforcement, and general representation of themselves to others. Our review process reflects that. When you sit on a review committee now, there are a variety of people who represent a domain. The group process on a review committee is different on two counts. One is that the nature of the knowledge each person represents is to a degree further developed and to another degree further fractionated from the person sitting on right or left, and all you have to do is sit on a review committee these days and you get the sense of the differences. Having chaired a review committee for a number of years, I know the function of the chairman is a crucial element in making people agree to agree, agree to communicate.

Now there are also those intangibles that are part of the review process. If—in the old days when I chaired a committee—you could fund down to 300, you could afford a fair amount of disagreement among the review members. But if you are not going to be able to fund below a 175 priority, you can't afford much disagreement. The committee processes reflect that problem. I don't think the review process has deteriorated in the sense that there is lack of total representation on committees. The question really is: Given a priority of 175 below which you can't fund, does that group process allow for somebody coming in with an innovative, slightly out of sync idea?

Now it happens that the process of integrating knowledge requires cutting across disciplines, particularly the little tiny distinctions we make among disciplines as we can see around this table. And when you do introduce an idea that cuts across these little fractionated areas of expertise and methodologic sophistication, you are endangering a priority score, if not an approval, depending on how that committee deals with the problem. In other words, the review process reflects in a sense the state of scientific knowledge in the broader society. The state of scientific knowledge in the broader society is highly developed, methodologically far more sophisticated, but fractionated. It is badly in need of integration around issues that society pays for with the hope of future payoff and fulfillment. So we are urged to get into applied research, to get into prevention, to do something that shows it is connecting up down the road

somewhere to better health. Now my point of view is that we need to deal with that. That is, we can't be satisfied with halfway measures for integration. We have to really deal with the need for designing new mechanisms to promote integrations across the disciplines and ultimately to get reviewers who are sophisticated about broader domains while focusing on a new narrow domain of research. It is fundamentally important in my view to learn more about the measuring of process with intrafamilial focus, to bring in an epidemiologic focus at a community level. But it is equally fundamentally important for all of you here to understand that mother-grandmother families may be just as important as mother-father, and that mother-alone families are quite different, and that we need to deal with those new kinds of data.

I think there is a social structural problem we have to deal with. It isn't sufficient to say that the integrating function comes from one of the three Institutes because it is politically viable that way and only that way. If that is the case, we should recognize where that brings us in relation to integration, and make mechanisms more relevant to where we are now, realizing these mechanisms we are using now were invented 30 years ago when things were quite different.

* * * *

STANTON: We shouldn't lose sight of the fact that we 10 persons were brought here in an experience and exercise that really has no precedent. This is the first such effort that I am aware of on this scale. I think we owe a debt to those who have invited us. There is the hopeful sign that our hosts have taken a meta view, or considerably more of a meta view, than we have seen in the past.

References and Notes

1. Coleman, Sandra B. Incomplete mourning and addict family transactions: A theory for understanding heroin abuse. In: Lettieri, D.J.; Sayers, M.; and Pearson, H.W., eds. *Theories on Drug Abuse: Selected Contemporary Perspectives.* National Institute on Drug Abuse, Research Monograph 30, DHHS Pub. No. (ADM) 80-967. Washington, D.C.: Supt. of Docs., U.S. Govt. Print. Office, 1980.
2. For persons who wish to look further into Dr. Langner's findings, two references to the Family Research Project are suggested:
   a. Langner, T.S. City children: The Family Research Project. In: Mednick, Sarnoff A. and Harway, Michele, eds., *Longitudinal Research in the United States,* Boston: Martinus Nijhoff, 1981.
   b. Langner, T.S.; McCarthy, E.D.; Gersten, J.C.; Simcha-Fagan, O.; Eisenberg, J.G. Factors in children's behavior and mental health over time: The Family Research Project. In: *Research in Community and Mental Health—An annual Compilation of Research,* Simmons, R.G., ed., January 1979, Part II, pp. 127-181.

   If copies are not obtainable, contact Dr. Langner at the following address:

   Dr. Thomas S. Langner
   Columbia University
   Division of Epidemiology
   600 West 168th Street
   New York, N.Y. 10032
3. National Institute of Child Health and Human Development, National Institutes of Health, Public Health Service, U.S. Department of Health and Human Services, Bethesda, Maryland.
4. Straus, Murray A., and Brown, Bruce W. *Family Measurement Techniques—Abstracts of Published Instruments, 1935-1974.* Minneapolis: University of Minnesota Press, 1978.
5. Baldwin, Wendy. Chief, Social and Behavioral Sciences Branch, Center for Population Research, National Institute of Child Health and Human Development, National Institutes of Health, Public Health Service, U.S. Department of Health and Human Services, Bethesda, Maryland.
6. Glick, P.C. Updating the life cycle of the family. *Journal of Marriage and the Family* 39:5-13, 1977.
7. Family Study Center, Department of Sociology, University of Minnesota, Minneapolis, Minnesota.
8. Gottman, John M. *Marital Interaction: Experimental Investigations.* New York: Academic Press, 1979.

☆U. S. GOVERNMENT PRINTING OFFICE: — 386-161

DEPARTMENT OF
HEALTH AND HUMAN SERVICES
PUBLIC HEALTH SERVICE

ALCOHOL, DRUG ABUSE, AND
MENTAL HEALTH ADMINISTRATION
5600 FISHERS LANE
ROCKVILLE, MARYLAND 20857

OFFICIAL BUSINESS
Penalty for private use, $300

POSTAGE AND FEES PAID
U.S. DEPARTMENT OF H.H.S.
HHS 396

THIRD CLASS
BULK RATE

NOTICE OF MAILING CHANGE

☐ Check here if you wish to discontinue receiving this type of publication.

☐ Check here if your address has changed and you wish to continue receiving this type of publication. (Be sure to furnish your complete address including zip code.)

Tear off cover with address label still affixed and send to:

Alcohol, Drug Abuse, and Mental Health Administration
Printing and Publications Management Branch
5600 Fishers Lane (Rm. 6C-02)
Rockville, Maryland 20857

DHHS Publication No. (ADM) 82-1213
Printed 1982